

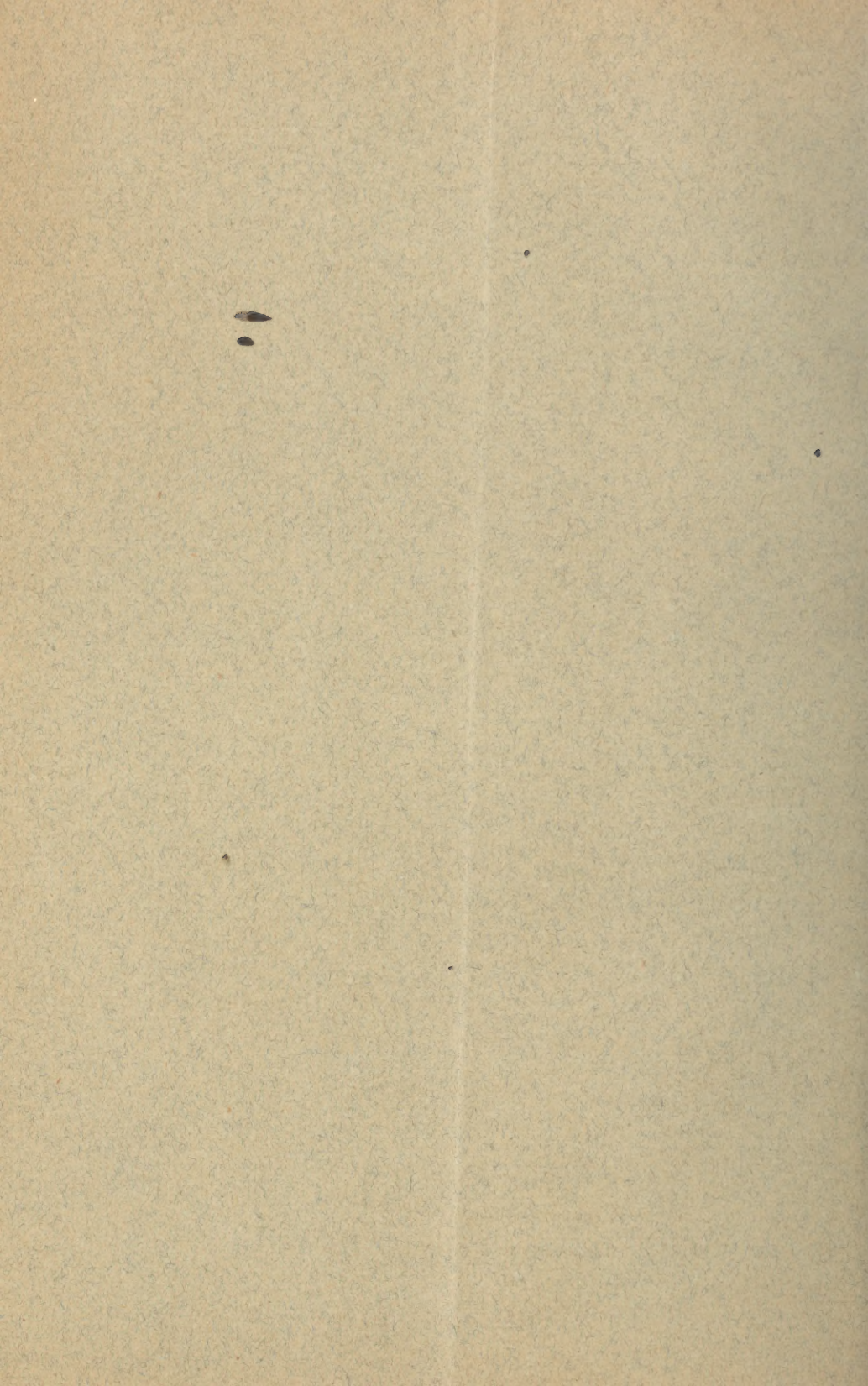
Duane (A.)

THE PARALLAX TEST FOR HETEROPHORIA.

BY

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In the summer of 1889 I published in the *New York Medical Journal* a description of what I denominated the parallax test for insufficiencies. Since that time a continuous experience has convinced me that in this test we have a method which is not only among the most satisfactory of all that we possess, but also in some cases succeeds when the other tests fail or are self-contradictory. As the test is not generally known, and as the only detailed account of it appeared in a general and not a special medical journal, it seems not without utility to rehearse briefly the method of its performance and the principles upon which it is based.

To conduct the test, we place the patient in the primary position, with head erect and eyes directed straight forward or slightly below the horizontal plane (this latter especially in making the test for near points). The object of fixation, which should be twenty feet distant, may be a candle-flame, but preferably is a white spot, 1 or 2 *cm* in diameter, upon a dull black surface of some considerable extent. By this arrangement all danger of projecting the image upon a surface beyond is done away with, and the chance of a confusion with surrounding objects is prevented. The patient's gaze being directed fixedly at the spot, a card is placed before one eye, and passed alternately from that to the other,—the patient being at the same time asked whether the spot appears to move, and, if so, in what direction. If it remains perfectly stationary, there can have been no deviation behind the

¹ Paper read before New York Academy of Medicine.



card, and the position of fixation of both eyes is perfect. If, however, the spot moves, it must occupy a different position as seen by the two eyes; *i.e.*, there is really a diplopia present which our method of observation has unmasked. Thus if, on uncovering the left eye, the object (which was previously seen by the right eye and is now seen by the left) appears to move to the patient's left, there is really a homonymous diplopia (homonymous parallax), which differs from ordinary diplopia only in the fact that the two images are seen alternately instead of at the same time; if the object seems to move to the right, there is crossed diplopia (crossed parallax); if the object moves down, the eye must have been higher behind the screen (left hyperphoria, left parallax); and if the object moves up, the left eye must have been lower behind the screen (right hyperphoria, right parallax). In order to determine the amount of this alternate diplopia, we place prisms of the appropriate direction and strength before one eye until the movement is abolished. Thus, supposing that when the left eye was uncovered the object seemed to move down and to the left (homonymous and left parallax, indicating a condition of hyperesophoria), two prisms are placed before this eye, with their bases respectively down and out, and increased in strength until the movement has become nil. The strength of the prism having its base down will measure the degree of hyperphoria, and that of the prism having its base out will indicate the degree of esophoria present.

For near points the test is made in the same way, a small dot on a rather large card being employed, and the movement of the dot upon the card (and not of the card itself projected against some distant object) being observed.

Such being the nature of the test, and such the simple means by which it is carried out, it remains to state the reasons for employing it at all. This is requisite, for we have already a number of methods of measuring heterophoria, and the question may naturally be raised why we should multiply tests when enough exist already for the purpose in hand. The answer to this is that the complexity of the problem presenting itself for solution is often such

that it is desirable to get confirmatory evidence of every kind before deciding, and, moreover, the other tests, while indicating the kind of heterophoria that is present, not infrequently give an erroneous impression as to its amount. Hence it is useful to have an additional source of information to supplement and check those which we have already; and it only remains to inquire if this method that we have described answers the demands that we can rightly make of it.

Of any test thus put forward we must ask how far it fulfils the requirements of applicability, precision, accuracy, and utility. As regards the first requirement, I may say that in my hands the test has proved to be almost universally applicable. There are very few persons who cannot with a little patience on the part of the examiner be got to recognize the displacement of the image, and to indicate its direction with accuracy. If the displacement is not recognized at once, it will in most cases be noticed after the patient's attention has been called to the artificial parallax produced when a prism of 2° or 3° is placed before the eyes. That is, the prism by exaggerating the parallax displacement has forced it upon the patient's attention, and has made him recognize in what it consists; and he is then able to say whether a similar movement does or does not exist when there is no prism before the eye.

As regards precision the test leaves nothing to be desired. A prism of $\frac{1}{2}^{\circ}$ suffices to produce or to neutralize quite a decided lateral parallax; and a vertical movement, corrected by a prism of even $\frac{1}{8}^{\circ}$, is clearly noticeable.¹

As regards accuracy, I have become convinced from the constancy of the results which the method affords that it is one of the most reliable of all that we have at our command. And this, in fact, constitutes its reason for existence; establishes its claim to be used with, and sometimes in preference to, the other methods of determining insuf-

¹ This precision makes it useful in determining whether a glass that the patient is wearing has or has not a prismatic element, and whether it is centered or not. If, for instance, a patient, who has no hyperphoria, shows, with his glass on, a vertical parallax, it proves either that the glass has a prismatic element in it, or is faultily adjusted (decentred) so as to produce a prismatic effect.

ficiency. A word upon this point will make this statement clear.

All the subjective tests for insufficiency depend in some way or other upon the production of diplopia. Usually this is effected by prisms, either applied roughly in the trial-frame, or with more precision in some one of the numerous varieties of phorometers. But the difficulty with the use of prisms has been that when two images of similar character are presented to the patient's notice, and he is required to state whether the two are or are not in the same vertical or horizontal line, he often unconsciously tries to bring them into line, and thus conceals an insufficiency which actually exists; or if the prisms are rotated, and the position of the images thus displaced, he instinctively tries to keep the images in line, and thus simulates an insufficiency which does not exist. This is often seen with the phorometer, and vitiates the findings of that otherwise most accurate instrument. Thus, when testing for near points, we are apt to find an exophoria, which increases almost indefinitely, although it is often quite certain that no such tendency to outward deviation actually exists. And while there are ways of obviating this objection that suggest themselves to the mind of the experienced examiner, they still constitute a source of uncertainty, particularly when the test is made for near points. Hence the attempt has been made to disassociate the images by giving one a different appearance from the other—*e.g.*, by converting a point of light into a luminous bar (Maddox rod), or into a blurred diffusion-circle (Stevens's spherical lens, with small aperture). But, as far as my experience goes, the impulse towards bringing two dissimilar images into line is almost as strong as when the images are alike. I believe that this is particularly the case with the Maddox rod. On account of this inaccuracy sometimes attaching to the subjective tests, some observers have preferred to rely solely upon objective methods, and have fallen back upon the old exclusion-test—*i.e.*, that in which the observer takes note of the deflection of one eye behind a screen, while the other eye is performing fixation. Now the parallax-test is

nothing but the subjective correlative of the objective exclusion-test, and is superior to the latter, both in being far more delicate and in admitting of precise measurement. Like the exclusion-test, it may be applied while the patient is looking in any direction whatever, and thus a difference in the amount of deflection in various parts of the field of fixation may be observed and estimated.¹ Moreover, the results are stabile, and when once the deflection is corrected, any further attempt at correction with prisms will produce a deflection in the opposite sense, so that there is not in this test, as in some of the others, a tendency on the part of the patient to overcome the prisms, and thus simulate an insufficiency which does not exist. In other words, the two images, being seen alternately, there is not the same impulse to force them together, as obtains in the other tests, where the two images are seen simultaneously. Hence it will often be found that when the phorometer indicates an insufficiency of the interni, or exophoria, of anywhere from 5° to 10° for near, the parallax test will show constantly a deviation outward of not more than 1° ; and corresponding to this small deviation it will be seen that the eyes remain steady behind the screen in performing alternate fixation.

Having used the test now for eight years, and upon a great number of cases, I am thoroughly convinced of its utility, and desire to recommend it to those who busy themselves with the consideration of anomalies of the ocular muscles. And whatever may be thought of the rôle that these anomalies play in producing direct or reflex disturbances, it is at least important that the data upon which our judgment of this difficult question is based should be as precise and as free from error as possible.

¹ I have already (ARCHIVES OF OPHTHALMOLOGY, xxiii., p. 71) signalized the importance of observing this difference, which may give us a clue as to the nature and etiology of the heterophoria.



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